The Impact of Private Equity Ownership on Medical Technologies

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Abstract

The impact of private equity (PE) ownership on long-run investments is theoretically ambiguous. PE ownership can provide capital and expertise that promote growth, but it may also redirect firms toward more certain, near-term improvements that predictably raise resale value, at the expense of riskier, long-run innovation. Yet, empirical evidence remains limited. This paper provides the first evidence of the effects of PE investment on medical technologies, a highly innovative sector in which significant PE activity has occurred. We build a novel data set of 416 medical device firms that received PE investment between 1995 and 2020 and match these to 916 never-treated comparison firms. Using a stacked difference-in-differences design, we estimate medium- and long-run impacts. The results imply that PE ownership reduces long-run patenting and product commercialization. These declines are concentrated in leveraged buyouts, which combine shorter investment horizons and substantial acquisition debt.

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1 Introduction

New product development requires firms to make uncertain, long-run investments. These investments span years of research and development (R&D), costly and uncertain regulatory review, and commercialization. Private equity (PE) ownership may weaken incentives to undertake long-run investments because PE funds typically operate on relatively short, predetermined horizons and, in the canonical leveraged buyout, place substantial acquisition debt on the target firm's balance sheet, constraining cash flow. Alternatively, PE acquisition may expand access to capital and technical and managerial expertise, which could accelerate growth and innovation and improve odds of success. Hence, the effects of PE investment on innovation are theoretically ambiguous. The limited empirical evidence on this topic, however, remains mixed.

We study the impact of PE acquisition in the U.S. medical device industry. This setting is economically important and well-suited for measuring product innovation. Medical device firms collected over $200 billion in annual revenue in 2019 (Donahoe 2021), and medical devices have played an important role in health improvements over recent decades (Cutler, Rosen, and Vijan 2006). In addition, among device manufacturers, innovation is crucial for maintaining a competitive advantage, as firms grow by developing new products and obtaining regulatory approval to market those products through the US Food and Drug Administration (FDA). Unlike many other industries, we are able to observe innovation beyond patents by using firm-level FDA submissions that detail commercialized products. The setting is also ideal for studying the impacts of PE acquisitions. PE investments in U.S. health care amounted to nearly $770 billion during 2013–2023 (Bain 2026). Although much of the current policy debate focuses on PE activity in other segments of the US health care sector, we find that PE firms acquired roughly 10% of innovative device firms operating in the U.S. during 1995–2020, a rate higher than the corresponding PE ownership rates for hospitals, nursing homes, and physician groups (Singh, Reddy, Papanicolas, and Scheffler 2026). These ownership transitions could change the rate and direction of socially valuable

product innovation, with consequences for providers, consumers, and payers. Yet, to our knowledge, no prior work has examined how PE ownership affects innovation in medical device technologies.

A simple conceptual framework helps illustrate the opposing mechanisms underlying the ambiguous effects of PE ownership on innovation. Building on Budish, Roin, and Williams (2015), we hypothesize that a finite investor horizon diminishes the net present value of long-run R&D when the exit price capitalizes only a portion of the unrealized pipeline value. Hence, all else equal, this factor will nudge a rational PE investor to reduce R&D investments relative to a non-PE investor. On the other hand, PE ownership could lower the cost of new product development by bringing technical expertise, improved R&D management, or reallocating resources toward more promising projects, thereby increasing the efficiency of investment. The net effect of PE ownership on innovation is therefore theoretically ambiguous.

We identify 610 firms targeted over 1995–2020 using proprietary information on deal records from PitchBook, Inc. We use also use PitchBook to identify medical device firms that appear in the private-capital data but never experience a private-equity buyout, and use these firms as never-treated controls. We obtain information on patent approvals from the US Patents and Trademarks Office (USPTO) and product applications from the FDA, which we then link to unique firms. These linkages allow us to build a novel panel dataset for approximately 6,200 distinct medical device firms, which we consider the near-universe of innovative firms in this industry.

To estimate the effects of PE investment, we use a stacked difference-in-differences (DD) design in which each treated firm is compared against up to three matched never-treated firms (Cengiz, Dube, Lindner, and Zipperer 2019). Our primary analysis sample contains 416 treated firms with pre-deal patents matched to 916 unique comparison firms (with replacement). Identification rests on the standard assumption that the treated and comparison firms would evolve in parallel in the absence of PE investment. Although this assumption is untestable, we find reassuring evidence that the treated and comparison firms trend in parallel in a

"testing" period prior to the window used to match firms.

Our central finding is that PE investment leads to a decline in long-run innovation. These declines typically emerge 5 or more years after the deal and progressively increase in magnitude. We report coefficients corresponding to the average treatment effects over years 0–6 (medium-run) and years 7–12 (long-run).[1] The long-run declines in patenting and FDA submissions are large. For example, we find that targeted firms file 15% fewer total patents and 14% fewer FDA submissions over years 7–12 after the deal. The declines are driven primarily by the extensive margin (i.e., firms are less likely to file any patent or FDA submission). These effects are both economically and statistically significant. In contrast, the medium-run effects are usually small and statistically insignificant. Taken together, PE acquisitions reduce measures of mid-to-late-stage innovation, materializing only after the typical PE holding period elapses.

The decline in innovation is concentrated in the subset of "leveraged buyout" (LBO) deals. Considered the canonical form of PE acquisitions, LBOs account for two-thirds of the deals in our sample. They have three distinct features, relative to non-LBO deals, that are likely to shape their effects on innovation. First, LBOs are typically financed with substantial debt. This debt, usually 50-70% of the deal value, is placed on the target firm's balance sheet, constraining its cash flow and potentially crowding out R&D spending. In contrast, non-LBO deals are often financed using the PE fund's own capital. Second, for LBOs the PE fund takes a controlling stake in the target firm, allowing more significant changes to strategy and operations than in non-LBO deals, where the PE fund typically takes a non-controlling stake. Finally, LBOs have shorter investment horizons than non-LBO deals (Kaplan and Strömberg 2009). For non-LBO deals, we find no statistically significant change in innovation, and the point estimates are small and *positive*. These results imply meaningful heterogeneity in the effects of PE investment that depends on the deal structure. A potential mechanism consistent with these findings is that the substantial debt used to fund LBO acquisitions

1. Year 7 marks the start of the long-run horizon as it coincides with the maximum gestation lag for new medical device products, consistent with the prior literature (Ji and Rogers 2024).

effectively raises product development costs due to debt servicing and crowds out long-run R&D investments.

A welfare interpretation requires caution. It is possible that PE owners cut redundant or low-return R&D projects. However, when we account for the number of citations each patent receives, a measure of the scientific value of innovation, our patenting estimates remain unchanged, suggesting that the marginal quality of innovation does not change. Additionally, industry-level estimates from Bloom, Schankerman, and Van Reenen (2013) suggest that the social value of medical innovation is at least twice as large as the private value, as medical device firms do not capture the full value of their technologies that accrue to patients or other firms (knowledge spillovers). Moreover, we find that acquired firms reduce medical patenting and patenting across all industries proportionally, suggesting that firms are not merely reallocating R&D efforts elsewhere. However, in this paper's current form, we do not measure other outcomes that PE acquisition could improve, such as firm survival and profitability.

The paper contributes to several intersecting strands of the literature. First, we add to limited evidence on the effects of PE investment on innovation. The closest study is Lerner, Sorensen, and Strömberg (2011) (LSS), who examine the impacts of LBOs across manufacturing-heavy industries and find no evidence of reduced patenting. They further show that post-buyout patents receive more citations. Although our results may appear to contradict their findings, important differences in the data and methodology may explain the divergence. First, we focus on medical device technologies, which is a small segment of their sample. Second, LSS does not use a DD design since they lack a never-treated comparison group. Instead, they focus on the change in innovation trends for treated firms before and after acquisition. Third, they focus on medium-run outcomes, following firms for five years after the deal. Indeed, we also find no decline in innovation output in the medium run; negative effects materialize only in the long run. More recently, Cumming, Peter, and Tarsalewska (2020) examine the effects of PE acquisition of publicly listed manufacturing

firms, which are quite different from the mostly private medical device firms in our sample, using a DD design and find a decline in both patent counts and citations. A related literature on PE's broader effects on operations, employment, productivity, and care quality (Davis et al. 2014; Bernstein and Sheen 2016; Bernstein, Lerner, Sorensen, and Strömberg 2017; Gupta, Howell, Yannelis, and Gupta 2024) provides a wider benchmark for interpreting our estimates.

Second, we contribute to the literature on medical innovation. Prior work has examined how regulatory pathway (Stern 2017; Grennan and Town 2020; Rogers 2024), intellectual-property scope (Budish, Roin, and Williams 2015; Williams 2013), demand and procurement (Finkelstein 2004; Clemens and Rogers 2026), and reimbursement design (Clemens and Gottlieb 2014; Dranove, Garthwaite, Heard, and Wu 2022; Ji and Rogers 2024) shape product development. These papers hold ownership fixed and vary policy; we hold policy fixed and vary ownership. The results show that firm ownership status affects the same invention and commercialization outcomes that the policy literature studies.

Finally, the paper speaks to short-termism and finite horizons. Stein (1989) formalizes the underinvestment problem under managerial myopia; Asker, Farre-Mensa, and Ljungqvist (2015) document that public firms under-invest in long-horizon projects relative to comparable private firms; Budish, Roin, and Williams (2015) show that finite patent terms distort pharmaceutical R&D away from late-stage cancer indications. We use PE acquisition as an observable shift in investor horizon within a single innovation-intensive industry. Holding technology and regulation fixed helps separate horizon-sensitive underinvestment from cross-sector composition.

The remainder of the paper proceeds as follows. Section 2 provides institutional background. Section 3 develops the conceptual framework. Sections 4 and 5 describe the linked dataset and the empirical strategy. Section 6 reports the main results. Section 7 concludes.

2 Background

This section describes the US medical-device industry, the structure of private-equity (PE) investment within it, and the deal-type variation in owner horizons that our empirical design exploits.

2.1 The US Medical-Device Industry

The US medical-device industry generates over $200 billion in annual sales and is among the more innovation-intensive segments of US health care. The Food and Drug Administration (FDA) regulates devices through a three-tier risk classification established by the 1976 Medical Device Amendments. Class I (low risk) devices generally require facility registration alone; Class II (moderate risk) devices typically proceed via the 510(k) "substantial equivalence" pathway; Class III (high risk) devices require premarket approval (PMA) supported by clinical evidence (Makower, Meer, and Denend 2010). The 510(k) pathway processes roughly three thousand submissions per year, while PMA originals number only thirty to fifty annually.[2] The two pathways differ in the type of innovation they reward: 510(k) activity is dominated by incremental product extensions, while PMA originals capture the breakthrough margin.[3]

A small set of large, publicly traded incumbents (Medtronic, Johnson & Johnson, Stryker, Boston Scientific) accounts for a sizable share of submissions, but the long tail of small and mid-size firms originates a large fraction of new device categories.[4] It is this long tail that supplies the population of PE-eligible targets.

2. Preparation of a PMA dossier routinely runs four to seven years from initiation, while a 510(k) submission can be prepared in months. Costs differ accordingly: Makower, Meer, and Denend (2010) estimates average costs of $24 million for 510(k) and $75 million for PMA.

3. Prior work on medical-device innovation has examined regulatory pathway design (Stern 2017; Grennan and Town 2020; Rogers 2024), intellectual-property scope (Budish, Roin, and Williams 2015; Williams 2013), demand and procurement shocks (Clemens and Rogers 2026), and reimbursement rules (Clemens and Gottlieb 2014; Dranove, Garthwaite, Heard, and Wu 2022; Ji and Rogers 2024). Prior coauthor papers on related questions include Rogers (2024), Clemens and Rogers (2026), Ji and Rogers (2024), and Gupta, Howell, Yannelis, and Gupta (2024). This paper complements that line by isolating the role of ownership rather than regulation, intellectual property, demand, or reimbursement.

4. In our analysis sample, the modal firm files one to three 510(k) submissions per year and maintains an active patent portfolio.

2.2 Private Equity in Medical Devices

PE investment in US medical-device firms expanded sharply between 1995 and 2020. PitchBook records over 1,400 PE-affiliated deals on US medical-device firms in our curated universe, with annual deal counts roughly tripling between the late 1990s and the late 2010s. Deals fall into four categories. *Buyout/LBO* pairs a controlling stake with substantial leverage and an active operating playbook (Kaplan and Strömberg 2009); sponsors aim to exit within a fund's investment horizon via secondary sale, strategic acquisition, or IPO. *PE Growth/Expansion* deals provide minority or non-controlling growth equity, carry little or no acquisition-related leverage, and typically come with board representation rather than operating control. *PIPE* (private investment in public equity) transactions inject capital into already-public firms.[5] *Investor Buyout by Management* captures management-led acquisitions with PE financing. Buyout/LBO and PE Growth together account for the large majority of medical-device deals.

The sectoral footprint tracks broader trends documented by Lerner, Sorensen, and Strömberg (2011): rising fund size and rising deal multiples.[6]

2.3 PE Ownership Horizons

PE owners hold portfolio firms for a finite, deal-type-specific period. Kaplan and Strömberg (2009) report a median holding period across US buyouts of roughly five years, with substantial right skew and few holds extending beyond ten years. The contrast between LBO and PE Growth holds is the within-PE source of variation in effective owner horizon that we exploit in Section 6.3. By contrast, independently owned firms—founder- or family-controlled firms, and publicly traded firms not in an imminent take-private transaction—carry no comparable horizon cap.

5. PIPE transactions behave more like financial than operating investments and constitute a small share of our sample.

6. See also Kaplan and Strömberg (2009) and Lerner, Sorensen, and Strömberg (2011) on the cross-industry evidence on PE and innovation. The medical-device sector is well suited to a within-industry test because deal flow is large and outcomes (patents, FDA submissions) are well measured at the firm level.

3 Conceptual Framework

We present a stylized model of innovation under finite ownership horizons in which firms choose a portfolio of R&D projects characterized by payoff horizon τ. The owner's effective time horizon T_θ varies by ownership type and truncates the value the owner captures from long-horizon projects. Similarly, owner types also differ in their development cost, $c(\tau, \theta)$, due to potential differences in managerial expertise and capital costs.

Projects differ in two primitives: their payoff horizon τ, defined as the time from initiation to commercial payoff, and their development cost $c(\tau, \theta)$. A line-extension or repackaged 510(k) submission may pay off in one to two years at modest cost; a novel implantable backed by a PMA may take seven to ten years and cost substantially more. Breakthrough projects—novel product categories, new therapeutic indications, first-in-class diagnostics—have longer mean horizons and higher development costs than incremental projects.

Owner types differ in the horizon over which they capture project returns. Specifically, non-PE owners effectively face no horizon cap ($T_{\text{non-PE}} = \infty$) since they do not need to sell their stake in the firm to realize profits. In contrast, PE sponsors hold their stakes for 5-7 years (Kaplan and Strömberg 2009). At the same time, it is plausible that some PE owners enjoy a lower development cost than non-PE owners at all gestation periods. That is, $c(\cdot, \text{PE}) \leq c(\cdot, \text{non-PE}) \; \forall \; \tau$. However, there may be heterogeneity on this dimension between LBO and non-LBO deals since the reliance on debt in the former constrains the firm's cash flow and raises the opportunity cost of deploying funds toward R&D. Therefore, it is possible that development cost is higher on average for PE funds in LBO deals than for the average non-PE owner.

At exit, the owner sells the firm and captures a fraction $\lambda \in [0, 1)$ of unrealized pipeline value through the sale price. We hypothesize that $\lambda < 1$, which reflects imperfect transmission of pipeline value through M&A markets: buyers discount unproven, long-horizon R&D more than realized cash flow, owing to information asymmetries about pipeline quality, integration risk, and exposure to the same horizon problem the seller faced. A risk-neutral owner

undertakes a project of horizon τ if

$$\mathrm{NPV}_\theta(\tau) \;=\; \int_0^{T_\theta} \pi(s)\, e^{-rs}\, ds \;+\; \lambda \int_{T_\theta}^{\infty} \pi(s)\, e^{-rs}\, ds \;-\; c(\tau, \theta) \;\geq\; 0, \tag{1}$$

where $\pi(s)$ is project cash flow at time s and r is the discount rate. When $\lambda < 1$ and T_θ is finite, a wedge opens between the owner's decision and that of a long-horizon planner, and the wedge widens in τ. However, if PE owners bring strategic and managerial expertise that create more efficient R&D processes, this advantage may offset the effect of the short investment horizon.

Equation (1) encapsulates the key forces at play when studying the effects of PE on long-run investment. It illustrates the ambiguity in predicting the net effect of PE ownership. As λ decreases in value, PE funds perceive a lower payoff from investments that have a long-run payoff and are less likely to invest in R&D. However, depending on their cost advantage, they may perceive a higher net present value and be more likely to invest. To the extent that PE funds face higher development costs in LBO deals, this simple framework predicts a decline in innovation in LBO deals relative to non-LBO deals, even holding the investment horizon constant.

4 Data

We assemble a firm-year panel that links PitchBook deal records, USPTO patent grants and applications from PatentsView, and FDA regulatory submissions for US medical-device firms over 1995–2024. Building this panel requires resolving heterogeneous firm-name strings across three independent administrative systems — none of which share a common identifier — and then constructing matched pairs of PE-acquired ("treated") and never-PE-acquired ("control") firms whose pre-period innovation trajectories are statistically indistinguishable. This section describes the three data sources (4.1), the entity-resolution procedure that maps firms across them (4.2), the matching procedure that selects the analysis sample (4.3), the

sample-flow restrictions that produce the headline estimation sample (4.4), and the pre-period balance the matched design achieves (4.5).

4.1 Sources

PitchBook. PitchBook is the source for the universe of PE deals and for firm metadata — founding year, location, deal history, and deal type. We use two PitchBook extracts. The first is the full deal record of 174,726 deals spanning 1980–2025, from which we draw the control pool. The second is a curated medical-device PE deal list containing 1,998 deals across 1,464 unique firms, last refreshed in October 2022; this defines our treated population. Treated firms are PE acquisitions of US medical-device firms that appear in PitchBook's innovative medical-device universe. Controls are drawn from the same universe but are not flagged as PE targets in the curated list. Deal types in the treated set include Buyout/LBO, PE Growth/Expansion, PIPE, and Investor Buyout by Management.

PatentsView (USPTO). PatentsView is the USPTO's bulk data repository of patent applications and granted patents. We extract filing date, grant date, CPC classifications, assignee identifiers, and forward citations. We align event time to filing year rather than grant year because filing dates are closer to the underlying R&D decision — grant lags average 2–3 years and are partly determined by examiner backlogs that have little to do with firm strategy. Forward citations are aggregated over a five-year window from grant and normalized within technology field and grant year following Hall, Jaffe, and Trajtenberg (2001) (the HJT normalization).

FDA. The FDA data comprise 510(k) substantial-equivalence submissions, PMA premarket approvals, MAUDE adverse-event reports, recall records, and 2009–2022 establishment registrations. 510(k) is the dominant clearance pathway for Class II medical devices; PMA covers higher-risk Class III devices. We define a firm-year regulatory submission as the count of 510(k) and PMA submissions on which the firm appears as either applicant or contract

manufacturer of record. Adverse-event reports come from the Manufacturer and User Facility Device Experience (MAUDE) database and are aggregated to the firm-year by manufacturer string. Establishment registrations are used only for entity resolution — they provide an additional set of firm-name strings to anchor the embedding cluster.

4.2 Entity Linkage

The three primary data sources use different firm-name conventions, and the same firm appears under many name variants in the FDA submissions, recalls, MAUDE, and registration files — legal entity names, doing-business-as names, subsidiary names, and registered trade names. Building a firm-year panel therefore requires us to resolve these strings to a common entity. This is a non-trivial step in the data construction; we summarize the procedure here.

We resolve 124,848 unique normalized firm-name strings to 76,572 distinct firm clusters using OpenAI's `text-embedding-3-large` model, which produces 3,072-dimensional vector embeddings of each name. We then build a k-nearest-neighbor graph on the embeddings using FAISS with $k = 50$, and cluster via a recursive single-link Union-Find procedure with iterative threshold tightening — raising the cosine threshold whenever a cluster grows pathologically large, which breaks chains formed by collisions on common aliases (for example, where two genuinely distinct firms share a parent name). The threshold is calibrated against a hand-labeled validation set of 2,000 firm pairs.

Each PitchBook firm — both treated and control — is mapped to its union of FDA embedding clusters via a GPT-validated alias graph. We build the alias graphs symmetrically for treated and control firms, so any alias accepted for a treated firm clears the same evidence threshold for controls. The symmetry is important: an asymmetric procedure would inflate the treated firms' FDA activity relative to controls and bias the post-deal comparison.

PatentsView assignee disambiguation is taken from the published PatentsView assignee files. We link PitchBook firms to PatentsView values via a separate alias graph that combines the PitchBook `companyname`, `companylegalname`, `companyformername`, and `companyalsoknownas`

fields with a fuzzy match against PatentsView assignee names. Match scores below the validated threshold are reviewed case by case.

Table 1 presents descriptive statistics for the universe of medical device firms produced as a result of this process. We show mean values separately for the 610 firms that received PE investment and the 5,628 never-treated firms that provide the comparison pool. The table clearly shows that PE funds target device firms that are more likely to file patents and submit new product applications to the FDA. However, never-treated firms have greater patent counts on average, suggesting they may be larger. We therefore use a matching procedure to define a subset of never-treated firms that are similar to the targeted firms. The next section describes the matching procedure.

4.3 Matching Procedure

For each treated firm T with deal-anchor year a_T, we select $K = 3$ never-PE-treated controls by minimizing a weighted distance over pre-period outcome trajectories, firm age, and pre-period patent stock:

$$d(T,C) \;=\; \sum_{o\in\mathcal{O}} w_o \left\| \tilde{y}^{T}_{o,[-5,-1]} - \tilde{y}^{C}_{o,[-5,-1]} \right\|^2 \;+\; \omega_{\text{age}}\left(\tilde{a}^{T} - \tilde{a}^{C}\right)^2 \;+\; \omega_{\text{stk}}\left(\widetilde{\log(1+s^{T})} - \widetilde{\log(1+s^{C})}\right)^2, \tag{2}$$

where $\mathcal{O} = \{\text{patents, A61, FDA submissions, late-stage VC, early-stage VC, M\&A}\}$ is the set of six pre-period outcome margins, $\tilde{y}^{T}_{o,[-5,-1]}$ denotes the $\log(1+\cdot)$-transformed and z-scored trajectory of outcome o for firm T over event-time $[-5,-1]$, a is firm age in years from the PitchBook `cp_foundyr` field, and s is cumulative pre-period patent stock. The outcome weights are $w_o = 1$ for all o; the side weights are $\omega_{\text{age}} = 10$ and $\omega_{\text{stk}} = 5$, both larger than w_o because age and stock are scalars rather than five-year vectors.

We enforce a hard caliper on technological proximity: the CPC subclass cosine (Jaffe 1989) at the deal-anchor year must be at least 0.50. The cosine is computed at the firm-pair level over portfolio shares in CPC subclasses (for patents) and FDA regulation numbers (for

FDA-mapped firms). Controls below the caliper are dropped from the candidate set before the distance minimization runs.

We match $K = 3$ with replacement. The choice trades variance reduction against the risk of admitting poor-quality matches; with replacement, a strong control can serve multiple treated firms rather than forcing weaker matches into the sample. Controls reused across stacks are handled in the estimation step through firm-by-stack fixed effects (Section 5.1). The match yields 610 unique treated firms paired with 1,173 unique never-treated controls, producing 1,830 paired stacks before any sample restriction.

4.4 Final Analysis Sample

Not every pair survives to the headline estimation sample. Two binding restrictions follow. First, we require both the treated firm and its matched control to have at least one US patent filed in the ten years preceding the deal anchor (event-time $[-10, -1]$); firms with no pre-period patenting have no patenting capacity to lose, offering a lower bound to the treatment effect on patenting. Second, FDA-side outcomes require both sides of the pair to have a non-empty FDA cluster mapping. Table A1 provides a flowchart of sample construction.

The headline sample is the row in bold: 416 unique treated firms matched to 916 unique controls, producing 1,205 paired stacks. Of the 610 treated firms after first-deal deduplication, 194 fail the patent-active filter — they had no US patent in the decade before the deal. Because we estimate every outcome on this single uniform sample, the headline estimates answer a specific question: what is the effect of PE ownership on medical-device firms with prior innovation output? The estimates are not the average effect across the full PE-acquired population. FDA-side outcomes apply the additional FDA-mapping restriction described above, which trims the sample to 281 treated $\times$ 616 controls.

4.5 Descriptive Statistics

Table 2 presents descriptive statistics on our main analysis sample using the same format and variables reported in Table 1. This table confirms that the matching procedure has helped find a subset of never-treated firms that are much closer to the treated firms on all attributes. They are of similar age, have similar levels of patenting and FDA applications, and have received investment from venture capital firms at similar rates prior to the deal year. Table A2 presents the distribution of product categories for the treated and matched never-treated firms and shows that the compositions of the two groups are very similar.

Table A3 lists the top 15 PE firms represented in our analysis sample (by number of deals). The firm with the most deals accounts for less than 3% of all deals. Hence, no single PE firm dominates our sample. We also classify the PE investors broadly as generalists or healthcare specialists based on their deal activity recorded in Pitchbook during 1995–2020. Investors with more than 50% of their deals involving a target in a healthcare-related industry (e.g., elder care, hospital, medical devices, biotech, etc.) are classified as healthcare specialists. Using this data-driven approach, the top firms are slightly more likely to be generalists (9 of 15) rather than focused on the healthcare sector. We further classify a deal as "health care specialized" if at least one of the PE investors involved in the deal is a specialist. This approach leads us to classify approximately 55% of deals as involving a specialist.[7] Table A4 presents descriptive statistics on the target firms in the two types of deals. Targets are similar across the two types of deals with two noteworthy differences. First, deals involving specialists target substantially younger firms (16 vs. 22 years in the deal year). Second, deals involving specialists are more likely to be leveraged buyouts. Later, we examine whether there is meaningful heterogeneity in the effects on innovation based on the composition of the PE investors.

7. 23 target firms (about 5%) belong to deals that cannot be classified into either category since we do not have information on the investors involved.

5 Empirical Strategy

5.1 Estimating Equation

For each outcome Y, we estimate

$$Y_{ist} = \beta_{medium} \cdot \mathbf{1}[\text{treated}_i \cdot t \in \{0,6\}] + \beta_{long} \cdot \mathbf{1}[\text{treated}_i \cdot t \in \{7,12\}] + \alpha_{is} + \gamma_{ts} + \varepsilon_{ist}, \quad (3)$$

where i indexes the firm, s indexes the matched stack, t is calendar year, and the indicator $\mathbf{1}[\text{treated}_i \cdot t \in \{0,6\}]$ equals one for the treated firm from the deal year onward through 6 years and zero otherwise. The coefficient β_{medium} identifies the average treatment effect over years 0 through 6 for treated firms under the conditional parallel-trends assumption. Similarly, β_{long} recovers the average treatment effect over years 7 through 12 after the deal year for treated firms.

Two features of the fixed-effect structure deserve emphasis. First, α_{is} is a *firm-by-stack* fixed effect, not a firm-only fixed effect. A control firm that is reused across multiple stacks enters each stack with its own fixed effect, so within-stack identification is not contaminated by cross-stack pooling. This is the standard stacked-DiD convention introduced by Cengiz, Dube, Lindner, and Zipperer (2019) and is essential whenever controls are reused across treated cohorts. Second, γ_{ts} is a *stack-by-year* fixed effect: each treated firm's counterfactual is the matched controls' value in the same calendar year within the same stack, so the comparison absorbs any within-stack same-year common shock. Standard errors are clustered on at the firm level, not the firm-by-stack composite, so that within-firm correlation propagates across the stacks in which a given control firm appears. We estimate Equation (3) on an event-time window of $[-12, +12]$.

For each outcome we report two ATT coefficients. β_{medium} ($t \in [0,6]$) and β_{long} ($t \in [7,12]$) are estimated jointly within a single two-indicator regression as shown in Equation (3). This approach allows us to test whether the post-deal effect grows over event-time.

5.2 Identification

Identification of the ATT rests on the conditional parallel-trends assumption: absent the PE acquisition, the matched controls' time path within the stack would be a valid counterfactual for the treated firm. This is untestable. However, we assess the plausibility of this assumption by presenting event study plots for all outcomes. For each outcome we estimate dynamic coefficients spanning the window [T-12, T+12]. The coefficients in the pre-treatment period help assess whether the treated and comparison firms were trending in parallel prior to treatment. In particular, coefficients for event years [-12,-6] allow us to assess whether the treated and comparison firms were trending in parallel since much before the deal and *prior* to the period whose data is used in matching.

Three design choices strengthen our confidence in the estimates in addition to the lack of differential pre-trends. First, we match target firms to untreated firms on five years of pre-period outcome trajectories across six dimensions: patents, A61 patents, FDA submissions, early-stage VC, late-stage VC, and M&A activity. This ensures that the comparison firms are very similar to the targets leading up to the deal year. Second, by matching on the patent subclass similarity score, we restrict matches to firms operating in similar technology areas. This addresses the concern that medical-device sector shocks, for example, a Center for Medicare and Medicaid Services (CMS) coverage decision affecting cardiovascular devices, could hit treated and control firms differentially if the matched control sits in a distant technology space. Third, the stack-by-year fixed effect γ_{ts} removes within-stack common shocks affecting treated and control firms in the same calendar year. The comparison in Equation (3) is between deviations from this within-stack year mean, not between levels.

6 Results

6.1 Patent Output: Quantity and Quality

Table 3 reports our main estimates using Equation (3) of the effects of PE investment on patenting (across all categories) by medical device firms. Each row reports coefficients for three outcomes estimated on the analysis sample of 416 treated firms matched to 916 unique comparison firms. We first examine the effect on patent counts. Since there is great variance in patenting levels across firms, we study the log of patent counts instead.[8] In addition to examining changes in unweighted patent counts, we also examine trends in counts weighted by 5-year citations, as discussed previously. We can then assess whether the marginal patents are of low or high quality. Finally, we also examine the probability of filing a patent. By comparing the effect on total patent counts with the effect on the extensive margin, we can say something about the role of the extensive versus intensive margins. For each outcome, we report effects in the medium run (0-6 years) and long run (7-12 years) to test whether the effects on innovation differ between these phases of ownership. Since medical device development cycles range from three to seven years for moderate-risk devices and longer for Class III breakthrough devices (Makower, Meer, and Denend 2010), it is possible that reductions in R&D investment by PE funds are apparent only in the long run.

The estimates in Table 3 indicate distinctly different effects at the two horizons. We find a small and statistically insignificant decrease in medium-run patent counts of -0.024. In contrast, the long-run estimate implies a 15% precisely estimated decline in patent count in years 7–12 relative to matched controls. When we weight patent counts by 5-year citations, the difference between medium- and long-run effects becomes even sharper. The medium-run effect is nearly zero and positive while the long-run effect remains about 14% and statistically significant. A similar decline between unadjusted and quality-adjusted patent counts rules out the possibility that PE firms cut back on low value patents and focus resources on high

8. We add 1 to patent counts to avoid missing values when taking the log.

value research. Because HJT normalization removes field-year compositional drift in citation rates, the citation-weighted decline is not driven by treated firms mechanically shifting away from citation-rich fields. The marginal long-horizon project that treated firms forgo appears to be at least as cited as the typical patent the firm continues to file. At the same time, this evidence does not imply that the remaining patents become lower quality. Column 3 shows a small imprecise decrease in the probability of filing a patent in the medium-run but a 10.6 percentage point decrease in the long-run, which represents a 17% decrease relative to the baseline probability of filing a patent. Taken together, the results in columns 1 and 3 imply that the decline in patenting is driven by the change on the extensive margin.

We study dynamics by estimating Equation (3) as an event study. Figure 1 plots the event-time coefficients around the deal year for each of the three outcomes. Pre-period coefficients are typically small and statistically indistinguishable from zero without a noticeable trend. Specifically, a joint test of significance of the coefficients in the testing period of (-12,-6) fails to reject the null hypothesis for all outcomes (p-values reported in Table 3). Post-deal, the coefficients are small and statistically insignificant through $t = 3$, become consistently large and negative beginning around $t = 4$, and are well below zero by $t = 7$. The gradual post-period divergence is consistent with reduced project initiation under finite-horizon ownership which becomes apparent only years later as the technology becomes patent-worthy.

We find quantitatively very similar patterns when we focus on medical device patents. The corresponding event study plots are presented in Figure A1 and the regression coefficients are presented in Table A5 in identical formats. In particular, the estimates imply that the decline on the extensive margin is driven entirely by medical device patents, which suffer a relatively larger decrease than other types of patents.

6.2 FDA Submissions

We turn from patents to FDA regulatory submissions, which capture the firm's downstream commercialization pipeline. The FDA panel further relies on the FDA cluster mapping,

leaving 281 unique treated firms matched to 616 unique controls. The outcome combines 510(k) substantial-equivalence applications with pre-market-approval submissions. Table 4 reports the corresponding results obtained by estimating Equation 3.

Estimates indicate the same long-run pattern of decline observed for patents. The medium-run coefficient is essentially zero for total counts and on the extensive margin. In contrast, the long-run coefficient implies a roughly 14% decline in total FDA submissions in years 7–12 after the deal. The corresponding decline in the probability of filing for FDA approval is 14.4 percentage points, 28% of the baseline mean filing rate. Both estimates are precisely estimated. Taken together, these coefficients again imply that targeted firms are likely to stop filing for new product approvals entirely rather than continue to apply at a reduced rate.

Figure 2 plots the corresponding dynamic effect coefficients. The pre-treatment coefficients are reassuringly small and insignificant. A joint test of significance of coefficients in the period (-12,-6) fails to reject the null hypothesis for both outcomes. Post-treatment coefficients are close to zero initially and gradually become increasingly negative.

6.3 Heterogeneity by Deal Type

The canonical PE buyout relies greatly on debt to fund the acquisition and is therefore known as a leveraged buyout or LBO, as discussed previously. LBO deals account for two-third of the deals in our sample, or 272 of 416 treated firms. The remaining deals are known as "growth equity" deals. These typically involve the PE fund taking a minority stake in the target and tend not to rely on debt to fund the investment. We stratify the sample into LBO and non-LBO subsets and estimate Equation 3 separately. Since matched comparison firms are anchored to specific treated firms, they are accordingly assigned to the stratified subsets. We examine heterogeneity in the effects on both patenting and FDA submissions.

It may help add context to the results to first describe differences between the firms targeted in LBO versus in non-LBO deals. Table A6 presents descriptive statistics comparing targets between the two types of deals. Firms targeted in LBOs are more mature (22 vs.

14 years old) and produce more patents (4.4 vs. 3.3) at the time of the deal. But they are not uniformly more innovative since they submit fewer product applications to the FDA. Table 5 reports the estimated coefficients separately for the two subsets on patenting. Panels A and B present the results on total patent counts and probability of filing, respectively. Although we report the medium-run coefficients as well, for brevity, we focus the discussion here only on the long-run coefficients. The estimates indicate that LBO deals entirely account for the long-run decline in patenting. In fact, for LBO deals we find a decline in patent counts and probability of filing even in the medium-run. In contrast, the estimated effects for non-LBO deals are statistically insignificant and typically *positive* in sign. Figure 3 presents the corresponding event study plots which confirm a sharp decline in patenting for LBO deals but a slight increase in patenting in non-LBO deals. Reassuringly, pre-trends remain small in both cases, and are particularly compelling for LBO deals.

The pattern of sharp difference in results between LBO and non-LBO deals repeats when we examine heterogeneity in the effects on FDA submissions, presented in Table 6. In this case, medium-run effects remain small and statistically insignificant even for LBO deals. This is intuitive since product applications are downstream of patents and therefore have a longer gestation cycle. Figure 4 presents the corresponding event study plots, which are consistent with the regression coefficients.

6.4 Heterogeneity by Investor type

PE firms and investment funds vary on several dimensions, some of which may be relevant to their effects on the innovation output of the target firms. One such dimension we investigate briefly is whether the PE firm specializes in healthcare sectors or is a generalist, an aspect we touched on in Section 4.5. PE firms that are well versed with healthcare markets may be more assertive in directing the target's business strategy, including deciding which product lines or R&D tracks to focus on. Hence, we anticipate that targets of specialized investors likely experience greater changes than those in deals which only involve generalists. Gupta,

Howell, Yannelis, and Gupta (2024) make this inference in the setting of nursing homes and report that the changes in nurse staffing are driven disproportionately by specialized PE investors.

Table 7 presents the corresponding results stratified by deals involving specialized investors (columns 1-2) and those only involving generalists (columns 3-4). We find much larger effects in deals involving specialist investors, where a decline in patenting is also detected in the medium run. In contrast, the effects for targets of deals without specialists are positive in the medium run and statistically insignificant at both horizons. Figure 5 presents the corresponding event study plots, which are generally consistent with the regression coefficients.

In unreported results, we estimate a richer model that includes an interaction term for LBO deals. This allows us to test whether the sharp distinction in effects between specialist and generalist investors prevails when we control for the deal type. The results indicate that even within LBO deals, targets of specialist investors exhibit a larger decline in patenting than those with generalist investors. However, the differences are not as sharp as in Table 7. Hence, some of the difference apparent in these results is due to the fact that specialized investors are more likely to feature in LBO deals.

6.5 Robustness Checks and Threats to Identification

We report results from two robustness checks for the long-run estimates in Table 8. Panel A repeats the baseline coefficients from the main results. Panel B presents results from a subsample limited to firms (both treated and comparison) for which we observe all 12 years post-deal. We therefore refer to this as a balanced panel subsample. Reestimating the models on this subsample helps test whether our main estimates are driven by changes in firm composition. The resulting coefficients are larger in magnitude across all five outcomes. This is intuitive since later years, which presumably have larger effects, receive greater weight in the balanced panel than in the main sample. These results reassure us that the long-run estimates in the main results are not an artifact of the unbalanced nature of the panel. Panel

C presents results from a larger sample without imposing the restriction that firms need to have filed a patent or FDA application prior to the deal year. Approximately a third of treated firms (194 out of 610) have no patent or FDA application prior to receiving PE investment and these are included in this analysis. Mechanically, innovation output in these firms cannot decline after treatment. Hence, we expect to find attenuated effects in this sample, as the table shows.

M&A masking of controls. Following Davis et al. (2014) we censor firm-year observations after any merger, acquisition, bankruptcy, or going-out-of-business event, so that post-acquisition activity attributed to a defunct firm does not contaminate the panel. We apply this masking symmetrically to treated and control firms. About 1.1% of matched controls — 10 of 916 unique firms — experience a LBO during the post-deal event window. The masking rule from Davis et al. (2014) does not censor these because LBO is not one of the M&A-end-event deal types in the original specification. We assessed sensitivity of the results to dropping these control firms. We find that the estimates do not materially change.

Control-pool construction. The control pool is the universe of medical-device firms in the PitchBook innovative-universe file that are not flagged as PE targets in the curated October 2022 list. We identified 30 firms (0.5% of the 5,628-firm control pool) that appear on the curated PE-deals list but are not flagged as targets in the universe file, likely owing to incomplete reconciliation of patent-assignee identifiers across the two PitchBook extracts. Of these, five are drawn into the matched control sample. We drop the 30 affected firms from the control pool in a sensitivity exercise; headline magnitudes are within rounding of the reported estimates.

Inference under control reuse. Because matching admits replacement, a given control appears in up to 10 stacks (median 1.6 across unique controls). The firm-by-stack fixed effect absorbs the level of each (firm, stack) occurrence; clustering on firm propagates within-firm

correlation across the stacks in which the firm appears. In unreported results, we assessed sensitivity to clustering at the firm-by-stack level and at calendar-year and found they are similar to the baseline estimates.

7 Discussion and Conclusion

This paper provides the first empirical evidence, to our knowledge, on the effects of private equity (PE) investment in medical device firms. We build a novel dataset to examine the effects on 610 firms that received PE investments between 1995 and 2020, approximately 10% of all device firms. Using a matched stacked difference-in-differences design, we find that PE investment results in large long-run declines in innovation output, measured using patents and FDA product submissions. The decline is primarily on the extensive margin, with targeted firms less likely to file a new patent or device for approval. Consistent with a simple conceptual framework, the decline in innovation output is not evident in the medium term due to the long gestation cycle of new product development. Importantly, the decline is entirely concentrated in leveraged buyout (LBO) deals, which involve greater use of debt to fund the acquisition and a shorter investment horizon. Targets of investors specializing in healthcare sectors exhibit greater declines in innovation output.

The effects on patents and device submissions reflect changes in innovation output; they do not reveal whether PE owners cut research projects because of finite horizon, debt constraint, managerial turnover, or operating refocus. Three features of the empirical pattern are jointly informative. First, the patent and FDA declines are concentrated in the long-run and are statistically zero in the medium-run. Second, the long-horizon decline is concentrated in the LBO subset of deals, with non-LBO point estimates close to zero and frequently positive. Third, in unreported results we find that the reduction in patenting is proportional across the firm's established and new technical areas. The mechanism most consistent with these three features is finite-horizon truncation: short-horizon ownership reduces the net present

value of long-payoff R&D projects at the project-selection stage (Budish, Roin, and Williams 2015), the project-selection effect appears in observed patents with a lag determined by the R&D cycle. We therefore think this is the most important economic mechanism.

It is worth briefly considering three additional mechanisms that may simultaneously operate in PE ownership. The first channel is the heavy use of leverage in acquisition. The concentration of innovation decline in LBO deals strongly suggests that the heavy use of leverage crowds out investment in R&D. In our conceptual framework this corresponds to a higher development cost due to debt servicing. We view leverage as the most important channel after finite-horizon truncation. Second, PE investors may change the top managers of the device firm after they gain control. New leaders may have different preferences regarding the research direction of the firm and may shift investment allocations, which could ultimately lead to a decline and shift in innovation output. However, we think this channel is less compelling since it does not necessarily predict a long-run decline in output, instead one may expect a short-run disruption due to changes in leadership. Third, changes in firm operations. PE firms are well known to make meaningful changes to their target firm's suppliers, contracts, product portfolio, marketing, and sales strategies. Such changes could lead to short-run disruptions to existing R&D pipelines and a decline in output. Finding a greater reduction in patenting by targets of specialized investors lends empirical support to this mechanism.

Although we have shed some light on the effects of PE investment in medical device firms, several avenues remain open for future work on this topic. We allude above to our limitation in precisely delineating the role of different mechanisms that operate simultaneously in this setting. With additional data on research inputs and variation in investment time horizons, one may have more success in separately quantifying the roles of leverage and finite-horizon truncation channels. We document the effect on product development, but another important aspect is product quality. Since these products are used to deliver health care services, changes in quality could meaningfully affect patient welfare. Examining the

downstream effects on patient health will help quantify one important avenue through which PE investment in medical device technologies affects society,

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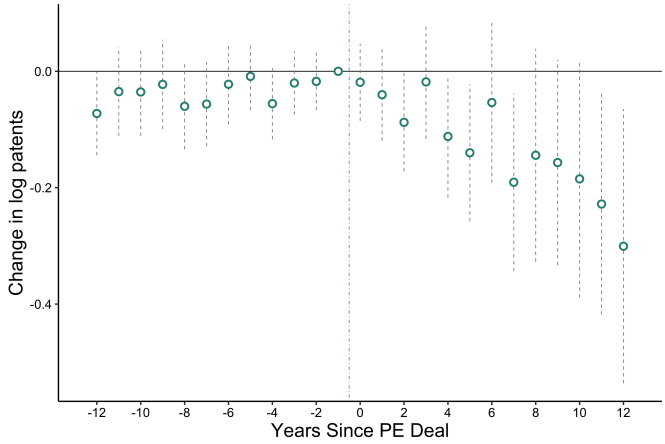
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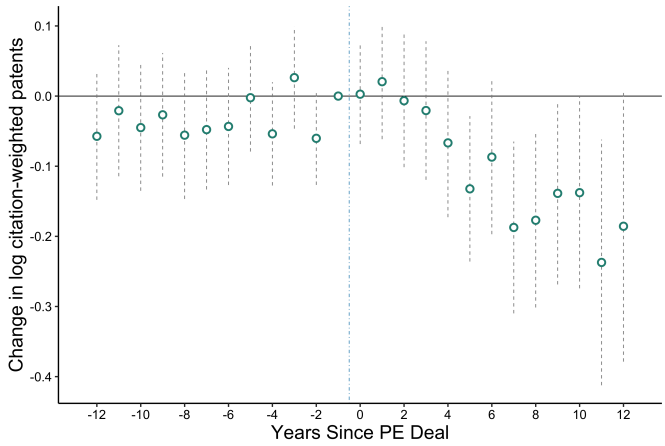
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(a) Patent (log) Count



(b) Citation-Weighted (log) Count



(c) Probability of Any Patents

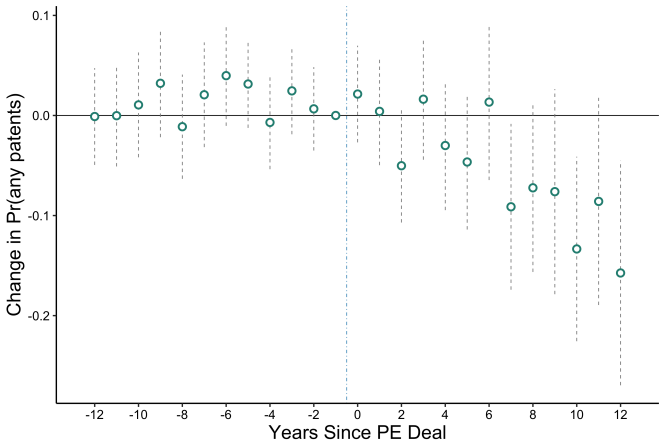
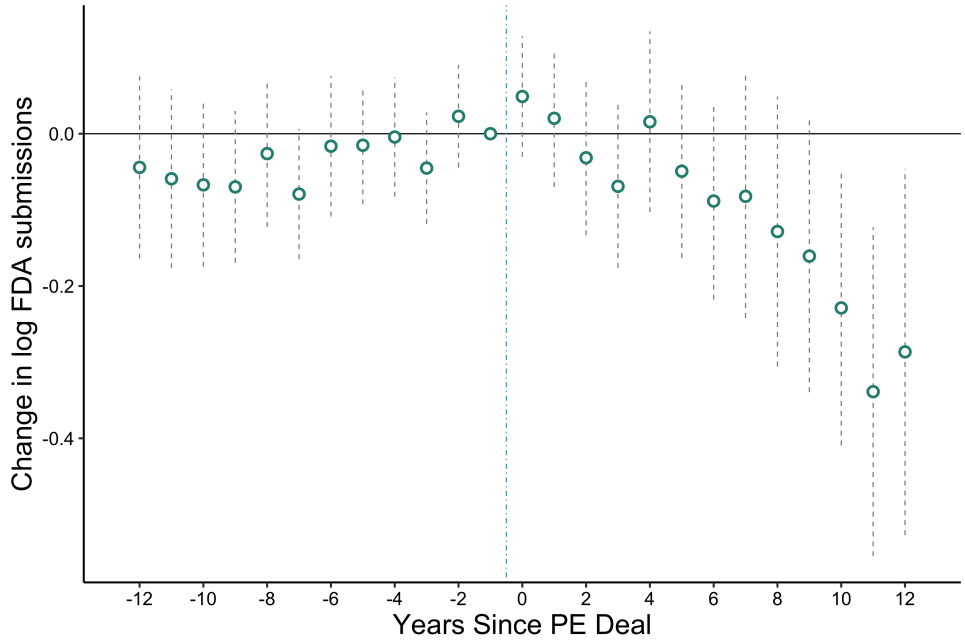


Figure 1: Effects on Total Patenting

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3). The outcome in Panel (b) is patent counts weighted by 5-year citation counts following the approach in Hall, Jaffe, and Trajtenberg (2001). The estimating sample includes 416 unique treated firms matched to 916 unique control firms (1,205 paired stacks). The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

(a) FDA (log) Submissions



(b) Probability of Any FDA Submission

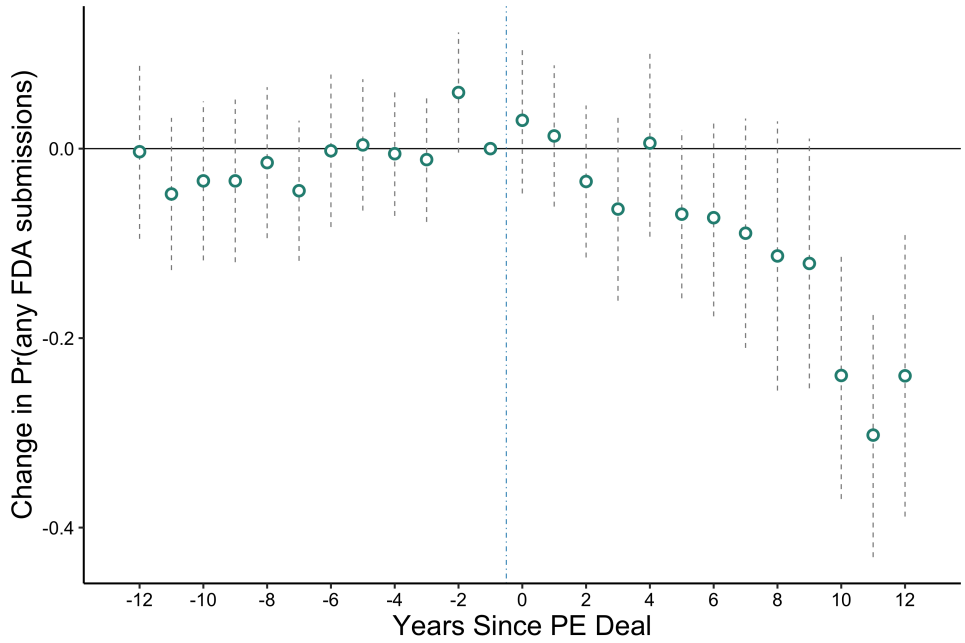
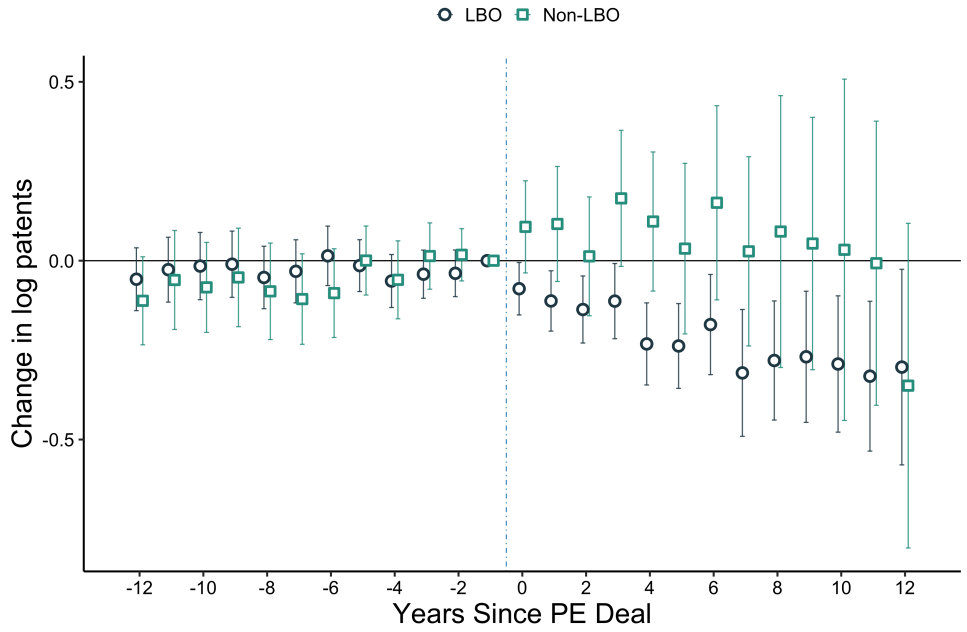


Figure 2: Effects on FDA Regulatory Submissions

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3). The outcome in Panel (a) pools 510(k) and pre-market approval submissions. Similarly, the outcome in Panel (b) is an indicator for any FDA submission. The estimating sample includes 281 unique treated firms matched to 616 unique control firms (1,020 paired stacks). The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

(a) Patent (log) Count



(b) Probability of Any Patent

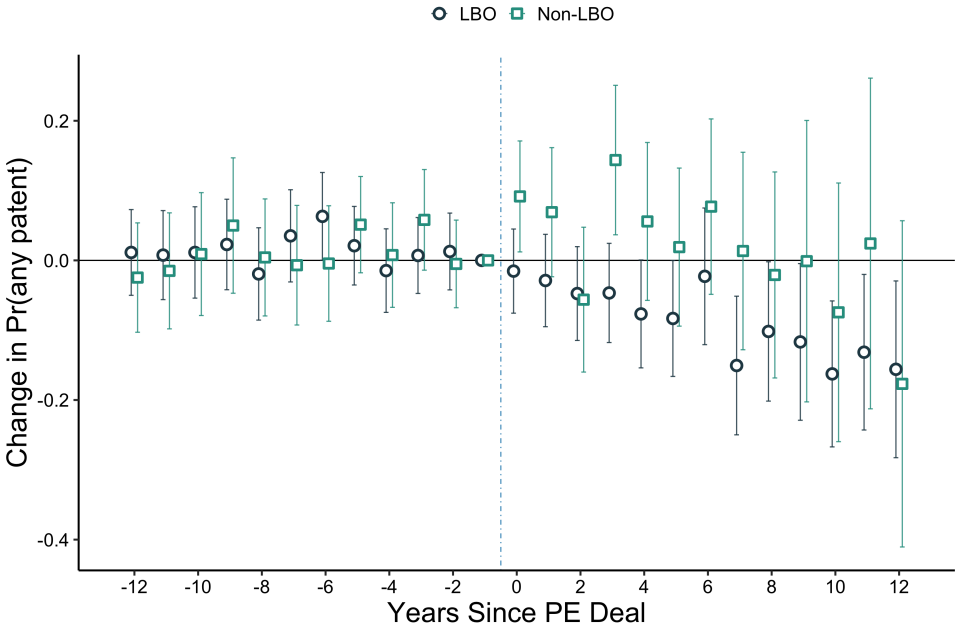
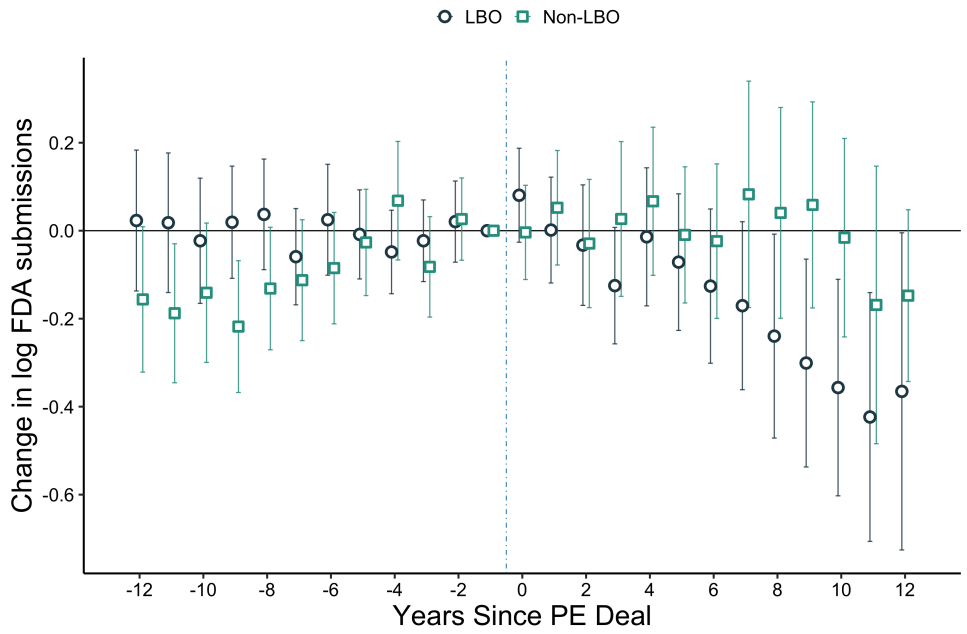


Figure 3: Heterogeneous Effects on Patenting by LBO Deal Type

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3), estimated separately for the LBO subset (272 treated firms) and the non-LBO subset (144 treated firms). The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

(a) (log) FDA submissions



(b) Probability of Any FDA Submission

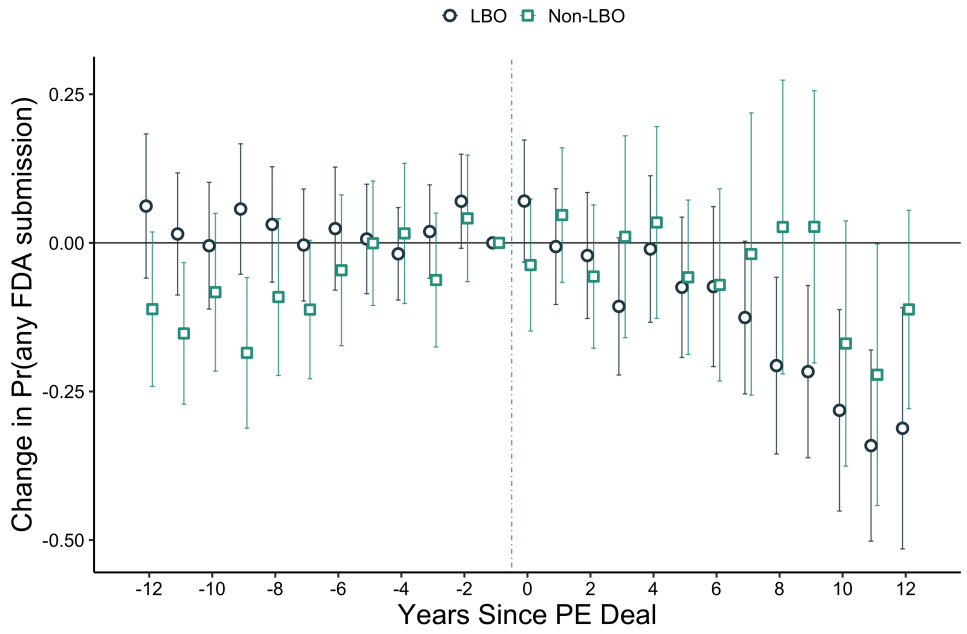
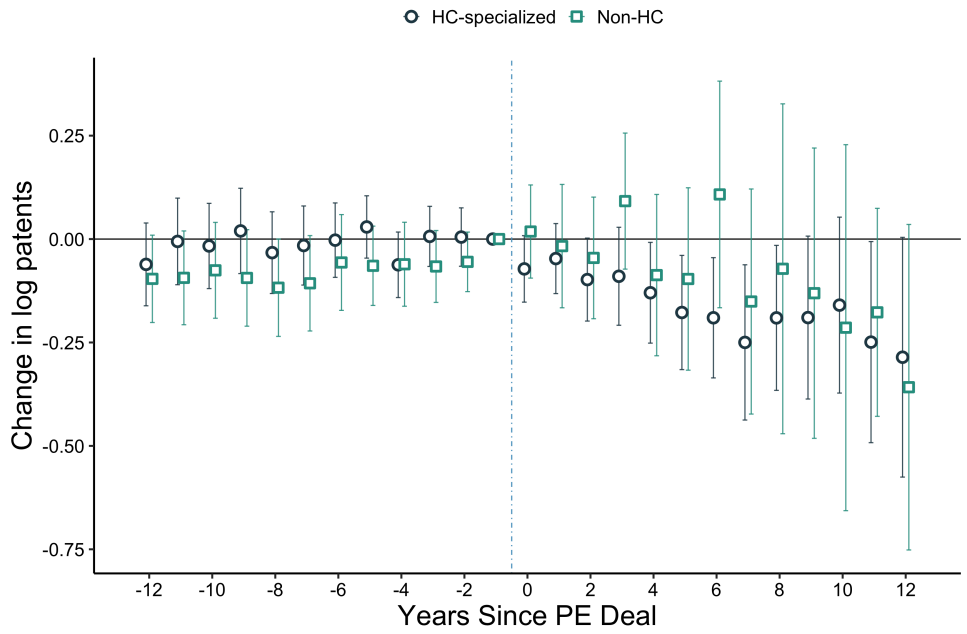


Figure 4: Heterogeneous Effects on FDA Submissions by LBO Deal Type

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3), estimated separately for the LBO subset (272 treated firms) and the non-LBO subset (144 treated firms). The outcome in Panel (a) pools 510(k) and pre-market approval submissions. Similarly, the outcome in Panel (b) is an indicator for any FDA submission. The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

(a) (log) Patent Count



(b) Probability of Patent Approval

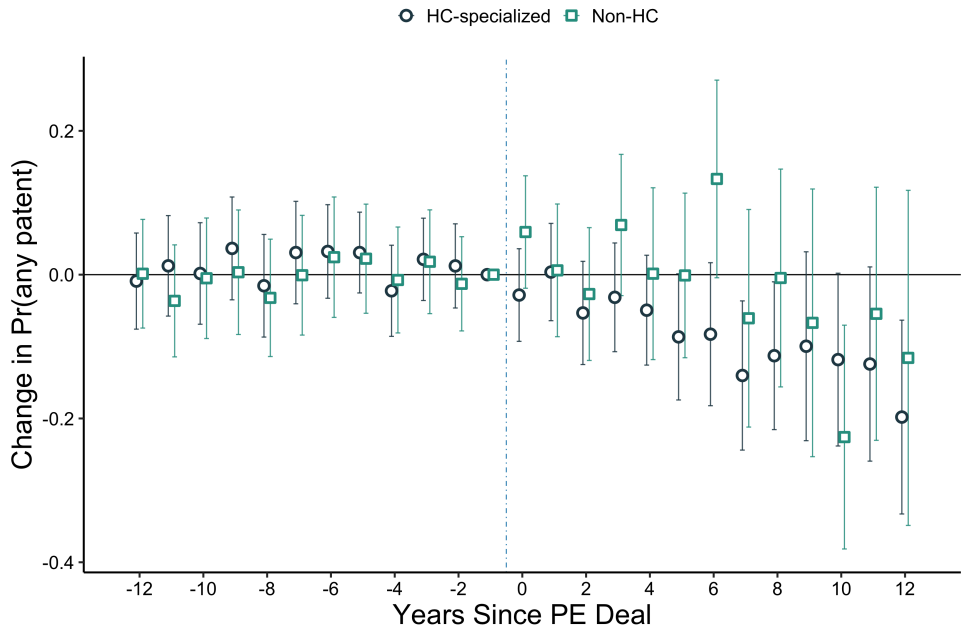


Figure 5: Heterogeneous Effects on Patenting by Investor Type

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3), estimated separately for the healthcare specialized subset (229 treated firms) and the generalist subset (164 treated firms). The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

Table 1: Descriptive Statistics, All Medical Device Firms

	Treated ($N = 610$)	Never-treated ($N = 5,628$)	Difference
A: Firm characteristics			
Mean firm age (years)	20.6	5.3	+15.3
Pr(any VC round in window)	0.14	0.15	−0.01
B: Patenting			
Mean patents per year	2.53	5.61	−3.08
Mean A61 patents per year	2.01	1.32	+0.70
Pr(any patent in window)	0.60	0.36	+0.24
Pr(any A61 patent in window)	0.50	0.29	+0.21
C: FDA submissions			
Mean FDA submissions per year	2.61	0.88	+1.74
Pr(any FDA submission in window)	0.35	0.17	+0.18

Note: The table presents descriptive statistics on the universe of medical device firms in our sample, stratified by PE investment status. Each observation is a unique firm. VC: venture capital, includes both early- and late-stage VC investments. A61 is the class corresponding to medical device technology patents. FDA submissions include both 510(k) and pre-market approval submissions. The statistics are calculated over a five-year window before the deal year for treated firms and the five years before the median treated anchor year (2010) for the never-treated sample.

Table 2: Descriptive Statistics, Matched Sample

	Treated ($N = 416$)	Matched comparison ($N = 1,205$)	Difference
A: Firm characteristics			
Mean firm age (years)	19.3	17.1	+2.2
Pr(any VC round in window)	0.17	0.15	+0.01
B: Patenting			
Mean patents per year	3.55	3.10	+0.45
Mean A61 patents per year	2.82	1.98	+0.84
Pr(any patent in window)	0.84	0.93	−0.09
Pr(any A61 patent in window)	0.70	0.77	−0.08
C: FDA submissions			
Mean FDA submissions per year	3.18	3.13	+0.05
Pr(any FDA submission in window)	0.41	0.33	+0.08

Note: The table presents descriptive statistics on the matched subset of medical device firms in our final analysis sample, stratified by PE investment status. The 1,205 comparison firm observations correspond to 916 unique firms since we match with replacement. VC: venture capital, includes both early- and late-stage VC investments. A61 is the class corresponding to medical device technology patents. FDA submissions include both 510(k) and pre-market approval submissions. The statistics are calculated over a five-year window before the deal year for treated firms. Matched comparison firms are assigned the deal year of their anchor treated firm.

Table 3: Effects on Patenting

	Log patents	Citation-weighted (log)	Extensive (any patent)
Medium $[0, 6]$	−0.024	+0.008	−0.019
	(0.042)	(0.038)	(0.020)
Long $[7, 12]$	-0.150^{*}	-0.137^{**}	-0.106^{***}
	(0.081)	(0.054)	(0.034)
Pre-period treated mean	4.03	5.28	0.580
Joint pre-trend p ($t \in [-12, -6]$)	0.18	0.76	0.36

Note: The table reports DD coefficients estimated using Equation (3) separately for the medium-run and long-run, which correspond to 0–6 and 7–12 years after the deal year, respectively. The analysis sample includes 416 treated firms and 916 matched unique comparison firms. Each cell reports the coefficient (standard error) from a single regression with both medium- and long-run indicators, firm-by-stack and stack-by-year fixed effects. The bottom row reports the p-value from a joint Wald test that the seven event-study pre-trend coefficients at $t \in [-12, -6]$ all equal zero. The matching window is $[-5, -1]$, so these far-pre coefficients are entirely out-of-sample for matching. Standard errors are clustered by firm to account for cross-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effects on FDA Product Applications

	Log FDA submissions	Extensive (any submission)
Medium $[0, 6]$	$+0.020$	-0.009
	(0.041)	(0.026)
Long $[7, 12]$	-0.136^{*}	-0.144^{***}
	(0.078)	(0.044)
Pre-period treated mean	2.83	0.391
Joint pre-trend p ($t \in [-12, -6]$)	0.45	0.52

Note: The table reports DD coefficients estimated using Equation (3) separately for the medium-run and long-run, which correspond to 0–6 and 7–12 years after the deal year, respectively. FDA submissions include both 510(k) and pre-market applications. The analysis sample includes 281 treated firms and 616 unique comparison firms. Each cell reports the coefficient (standard error) from a single regression with both Medium and Long indicators, firm-by-stack and stack-by-year fixed effects. The bottom row reports the p-value from a joint Wald test that the seven event-study pre-trend coefficients at $t \in [-12, -6]$ all equal zero. The matching window is $[-5, -1]$, so these far-pre coefficients are entirely out-of-sample for matching. Standard errors are clustered by firm to account for cross-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneity by Deal Type on Patent Output

	LBO		Non-LBO	
	Medium $[0, 6]$	Long $[7, 12]$	Medium $[0, 6]$	Long $[7, 12]$
A: (Log) patents				
ATT	-0.111^{***}	-0.262^{***}	$+0.144$	$+0.066$
	(0.042)	(0.083)	(0.089)	(0.167)
B: Pr(any patent)				
ATT	-0.054^{**}	-0.146^{***}	$+0.049$	-0.029
	(0.024)	(0.039)	(0.031)	(0.062)

Note: The table reports DD coefficients estimated using Equation (3) on samples stratified by whether the deal is a leveraged buyout (LBO) or not. For each sample, we estimate medium- and long-run coefficients, which correspond to 0–6 and 7–12 years after the deal year, respectively. The LBO subset contains 272 treated firms and 622 unique comparison firms. The non-LBO subset contains 144 treated firms and 370 unique comparison firms. Each cell reports the coefficient (standard error) from a single regression on the indicated subsample with both medium and long indicators, firm-by-stack and stack-by-year fixed effects. Standard errors are clustered by firm to account for across-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneity by Deal Type on FDA Submissions

	LBO		Non-LBO	
	Medium $[0, 6]$	Long $[7, 12]$	Medium $[0, 6]$	Long $[7, 12]$
A: (Log) FDA submissions				
ATT	-0.027	-0.273^{***}	$+0.100^{*}$	$+0.098$
	(0.054)	(0.105)	(0.060)	(0.089)
B: Pr(FDA submission)				
ATT	-0.044	-0.239^{***}	$+0.051$	$+0.017$
	(0.031)	(0.044)	(0.045)	(0.077)

Note: The table reports DD coefficients estimated using Equation (3) on samples stratified by whether the deal is a leveraged buyout (LBO) or not. For each sample, we estimate medium- and long-run coefficients, which correspond to 0–6 and 7–12 years after the deal year, respectively. FDA submissions include all 510(k) and pre-market applications. The LBO subset contains 174 treated firms and 413 unique comparison firms. The non-LBO subset contains 107 treated firms and 257 unique comparison firms. Each cell reports the coefficient (standard error) from a single regression on the indicated subsample with both medium and long indicators, firm-by-stack and stack-by-year fixed effects. Standard errors are clustered by firm to account for across-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity by Investor Type on Patenting

	Healthcare-specialized		Generalist	
	Medium $[0, 6]$	Long $[7, 12]$	Medium $[0, 6]$	Long $[7, 12]$
A: (Log) patents				
ATT	-0.087^{*}	-0.200^{**}	$+0.070$	-0.079
	(0.048)	(0.089)	(0.077)	(0.160)
B: Pr(any patent)				
ATT	-0.049^{*}	-0.135^{***}	$+0.032$	-0.072
	(0.026)	(0.042)	(0.031)	(0.061)

Note: The table reports DD coefficients estimated using Equation (3) on samples stratified by whether the deal has any healthcare specialized investor or not. For each sample, we estimate medium- and long-run coefficients, which correspond to 0–6 and 7–12 years after the deal year, respectively. The specialist and generalist subsets contains 229 and 164 treated firms, respectively. 23 treated firms cannot be classified to either group since we cannot observe the identity of their investors. Each cell reports the coefficient (standard error) from a single regression on the indicated subsample with both medium and long indicators, firm-by-stack and stack-by-year fixed effects. Standard errors are clustered by firm to account for across-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Robustness Checks

	Log patents	Cite-weighted (log)	Pr(Any patent)	Log FDA submissions	Pr(FDA submission)
A: Baseline					
Long $[7, 12]$ ATT	-0.150^{*}	-0.137^{**}	-0.106^{***}	-0.136^{*}	-0.144^{***}
	(0.081)	(0.054)	(0.034)	(0.078)	(0.044)
# treated firms	416	416	416	281	281
# paired stacks	1,205	1,205	1,205	1,020	1,020
B: Balanced subsample ($\geq$ 12 post-years)					
Long $[7, 12]$ ATT	-0.189^{*}	-0.145^{*}	-0.122^{***}	-0.135	-0.144^{***}
	(0.101)	(0.081)	(0.044)	(0.101)	(0.054)
# treated firms	140	140	140	152	152
# paired stacks	393	393	393	441	441
C: No patent filter sample					
Long $[7, 12]$ ATT	-0.054	-0.030	-0.053^{**}	-0.083	-0.099^{***}
	(0.050)	(0.037)	(0.025)	(0.055)	(0.033)
# treated firms	578	578	578	379	379
# paired stacks	1,827	1,827	1,827	1,526	1,526

Note: The table presents results from tests of robustness to changing the analysis sample. Each cell reports the long $[7, 12]$ coefficient (standard error) for each outcome from a single regression with both medium $[0, 6]$ and long $[7, 12]$ post-deal indicators, firm-by-stack and stack-by-year fixed effects, following Equation (3). Standard errors are clustered by firm as usual. Panel A repeats the baseline coefficients for ease of comparison. Panel B restricts to the cohort-balanced subsample in which the treated firm has at least twelve observable post-deal years. This drops younger deal cohorts and isolates magnitudes from younger-cohort dilution. Panel C estimates using all unique treated and matched comparison firms without imposing the restriction that they have at least one patent or FDA submission filed in the ten-year period prior to the deal year. Panel C estimates should be interpreted as a conservative lower bound in absolute value since roughly one-third of the treated firms have zero patents or zero FDA submissions in the pre-period, which mechanically cannot generate negative post-deal coefficients (their outcome can only remain at zero or rise). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additional Figures

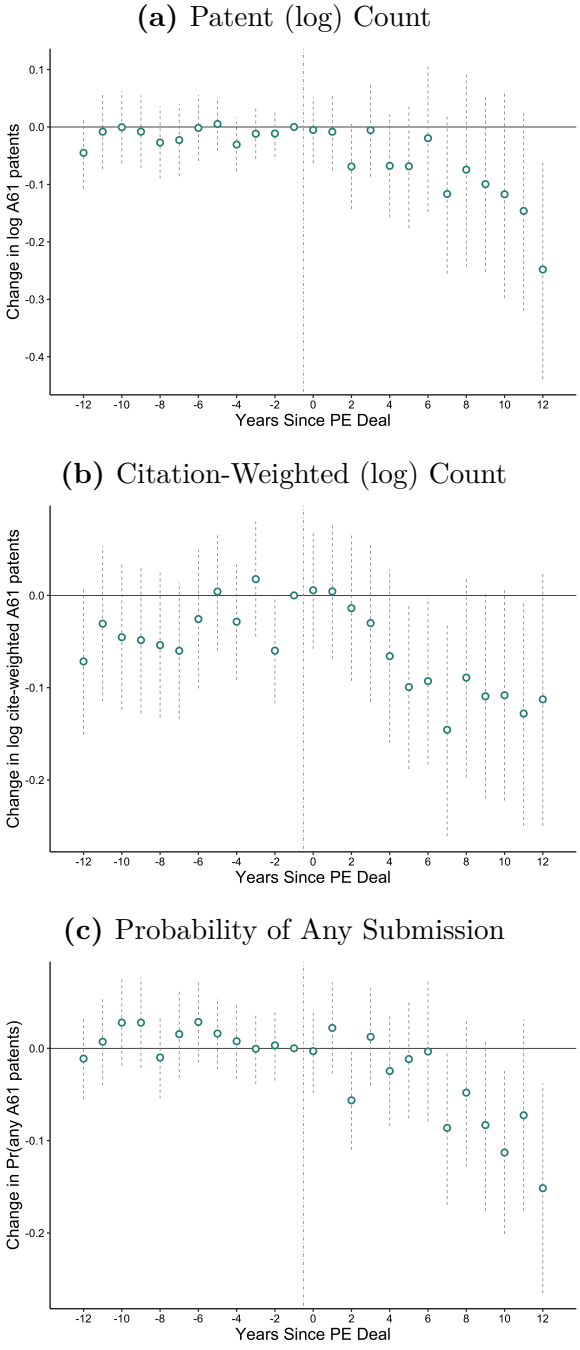


Figure A1: Effects on Medical Device Patenting

Note: This figure plots dynamic effect coefficients from the event-study version of Equation (3). It is similar in format to Figure 1 except that the outcomes correspond specifically to medical device patents, i.e., those filed in category A61. The outcome in Panel (b) is patent counts weighted by 5-year citation counts following the approach in Hall, Jaffe, and Trajtenberg (2001). The estimating sample includes 416 unique treated firms matched to 916 unique control firms (1,205 paired stacks). The coefficient β_{-1} is omitted and serves as the reference period. Standard errors are clustered by firm to account for cross-stack correlations within firm. Dashed grey lines indicate 95% confidence intervals.

Additional Tables

Table A1: Sample Construction Details

Stage / Filter	Treated deals	Treated firms	Control firms	Paired stacks
PitchBook medical-device innovative universe	—	728	—	—
Never-PE-treated control pool	—	—	5,628	—
First-deal deduplication (one deal per firm)	583	610	—	—
$K = 3$ NN match (with replacement)	583	610	1,173	1,830
Patent-active filter: both sides ≥ 1 patent in $[-10, -1]$	—	**416**	**916**	**1,205**
FDA panel (both sides FDA-mapped)	—	281	616	1,020

Note: The table provides details of how sample size changes as we impose additional restrictions to arrive at the analyses samples for various outcomes. The top 2 rows present the number of unique treated and never-treated firms, respectively, in our universe of medical device firms. Deal deduplication eliminates repeat instances of a firm receiving PE investment so we focus on the first treatment per firm. The patent-active filter is the binding constraint, where we limit to the subset with at least one patent in the ten years prior to the deal year. This sample is used in the patents analysis. FDA outcomes use a further subset which also has information on FDA product submissions.

Table A2: Product Category Composition

Category (FDA "Part")	% Treated	% Control
General Hospital and Personal Use	11.16%	9.89%
General and Plastic Surgery	10.84%	10.90%
Cardiovascular	10.42%	12.07%
Gastroenterology-Urology	9.26%	8.72%
Neurological	7.58%	8.02%
Orthopedic	7.58%	7.48%
Anesthesiology	6.53%	5.61%
Radiology	5.79%	7.87%
Physical Medicine	5.58%	4.21%
Ear, Nose, and Throat	4.42%	2.96%
Dental	4.00%	4.36%
Hematology and Pathology	3.79%	3.66%
Obstetrical and Gynecological	3.79%	3.58%
Clinical Chemistry and Clinical Toxicology	3.58%	4.36%
Ophthalmic	2.95%	3.27%
Immunology and Microbiology	2.74%	3.04%
Total	100.00%	100.00%

Note: The table presents distributions of the treated and matched comparison firms across different medical device product categories ("Part") used by the FDA to classify its submissions. Each column is normalized so it sums to 100%; firms can be active in multiple Parts so column totals exceed the underlying firm counts. Counts are unique firms with ≥ 1 deduplicated 510(k) submissions in the respective cell. The sample for this distribution is the FDA-mapped subset (281 unique treated firms; 616 unique controls); 135 of the 416 headline treated firms are not FDA-mapped and therefore absent from this table. Underlying count totals: 950 treated firm-Part observations, 1,284 control firm-Part observations.

Table A3: Top PE Firms by Deal Count

		Headline sample		Full design	
Investor	HC-specialized	# deals	%	# deals	%
RoundTable Healthcare Partners	Yes	10	2.6%	17	2.9%
Warburg Pincus	No	9	2.3%	12	2.1%
HealthpointCapital	Yes	8	2.1%	9	1.5%
The Riverside Company	No	8	2.1%	8	1.4%
Audax Private Equity	No	7	1.8%	9	1.5%
DW Healthcare Partners	Yes	7	1.8%	7	1.2%
Linden (Chicago)	Yes	5	1.3%	8	1.4%
Ampersand (Massachusetts)	Yes	5	1.3%	6	1.0%
Apax Partners	No	5	1.3%	5	0.9%
Cortec Group	No	4	1.0%	8	1.4%
Dempsey Ventures	Yes	4	1.0%	8	1.4%
Galen Partners	No	4	1.0%	6	1.0%
One Equity Partners	No	4	1.0%	6	1.0%
Riverside Partners	No	4	1.0%	6	1.0%
TPG	No	4	1.0%	6	1.0%

Note: The table lists the top 15 PE firms by the number of distinct deals in the patents analysis sample corresponding to 416 treated firms and 383 deals. Counts and shares are also reported for the unrestricted sample of 610 treated firms and 583 unique deals. Importantly, both samples have the same firms at the top, although the rankings are slightly different. We also indicate whether the PE firm specializes in healthcare sectors or not. A firm is classified as specialist if more than 50% of its deals during 1995–2020 involve a target in a healthcare sector. The denominator for the shares in each column is the number of total deals in that sample. A deal can be attributed to multiple investors (co-investments and syndicates) so column sums exceed 100% but accurately reflects a PE firm's participation share of all deals.

Table A4: Treated Firms by Investor Type

	HC-specialized ($N = 229$)	Generalist ($N = 164$)	Difference
Mean patents per year	3.44	3.19	+0.25
Pr(any patent in window)	0.95	0.95	−0.00
Mean A61 patents per year	2.59	2.51	+0.09
Pr(any A61 patent in window)	0.79	0.78	+0.01
Mean FDA submissions per year	2.34	3.28	−0.94
Pr(any FDA submission in window)	0.65	0.67	−0.02
Mean firm age at deal (years)	16.6	22.4	−5.75
% LBO deal type	72.3	61.6	+10.64

Note: Pre-deal observable characteristics of PE-acquired (treated) firms, split by whether the deal's investor pool included at least one healthcare-focused investor. Pre-deal window: five years before the deal anchor. Healthcare-specialized deals are tilted toward LBO (72% vs. 62%); the simple correlation between the LBO and Healthcare-Specialized indicators is 0.11. The HC subset spans 229 unique treated firms; the generalist subset spans 164. The remaining 23 of 416 active treated firms have no PitchBook investor information and are excluded from both subsets.

Table A5: Effects on Medical Device Patenting

	Log A61 patents	A61 cite-weighted (log)	A61 extensive (any)
Medium $[0, 6]$	−0.016	+0.004	−0.017
	(0.037)	(0.032)	(0.017)
Long $[7, 12]$	−0.103	-0.079^{*}	-0.094^{***}
	(0.074)	(0.044)	(0.032)
Pre-period treated mean	3.22	1.93	0.470
Joint pre-trend p ($t \in [-12, -6]$)	0.33	0.35	0.29

Note: The table reports DD coefficients estimated using Equation (3) separately for the medium-run and long-run, which correspond to 0–6 and 7–12 years after the deal year, respectively. The outcomes are similar to those in Table 3, except that we focus exclusively on medical device (A61) patents. The analysis sample includes 416 treated firms and 916 matched unique comparison firms. Each cell reports the coefficient (standard error) from a single regression with both Medium and Long indicators, firm-by-stack and stack-by-year fixed effects. The bottom row reports the p-value from a joint Wald test that the seven event-study pre-trend coefficients at $t \in [-12, -6]$ all equal zero. The matching window is $[-5, -1]$, so these far-pre coefficients are entirely out-of-sample for matching. Standard errors are clustered by firm to account for cross-stack correlations within firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Treated Firms by Deal Type: LBO vs. Non-LBO

	LBO ($N = 272$)	Non-LBO ($N = 144$)	Difference
Mean patents per year	4.44	3.26	+1.18
Pr(any patent in window)	0.94	0.96	−0.02
Mean A61 patents per year	3.54	2.61	+0.93
Pr(any A61 patent in window)	0.76	0.86	−0.10
Mean FDA submissions per year	2.67	3.10	−0.43
Pr(any FDA submission in window)	0.64	0.68	−0.03
Mean firm age at deal (years)	21.9	13.6	+8.2

Note: Pre-deal observable characteristics of PE-acquired (treated) firms, split by deal type. The LBO subset comprises Buyout/LBO and Investor Buyout by Management deals; the Non-LBO subset comprises PE Growth/Expansion and PIPE deals. Pre-deal window: five years before the deal anchor. The two subsets partition the sample exactly (LBO: 272 unique treated firms, 789 stacks; Non-LBO: 144 unique treated firms, 416 stacks).